

Comment on the proposed conservation of the specific name *Krynickillus maculatus* Kaleniczenko, 1851 (currently *Limax maculatus*; Gastropoda, Stylommatophora, LIMACIDAE)

(Case 3639; see BZN 70: 218–220)

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This is a rare case of a species that has two different correct names when placed in different genera: *Limacus maculatus* (Kaleniczenko, 1851) if placed in the genus *Limacus*, and *Limax (Limacus) ecarinatus* Boettger, 1881 if placed in the genus *Limax*. Some authors in the recent past have used *Limax* Linnaeus, 1758; others have used *Limacus* Lehmann, 1864 as the genus for this species. The name *Limax maculatus* (Kaleniczenko, 1851) is incorrect. This situation is undesirable. Balashov suggests suppressing the senior homonyms of *Limax maculatus*, so that *Limax maculatus* (Kaleniczenko, 1851) becomes a correct name. Alternatively the name *Limax ecarinatus* could be used for the species, in the form *Limacus ecarinatus* (Boettger, 1881) if placed in the genus *Limacus*, but in this solution *K. maculatus* Kaleniczenko, 1851 would have to be suppressed. For both solutions the Commission would have to decide. I support Balashov's proposal.

Comment on *Phoronis* Wright, 1856 (Phoronida) and *P. muelleri* Selys Longchamps, 1903: proposed conservation of both names

(Case 3626; see BZN 70: 157–159, 249)

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I write to point out some inexact and missing data in this application. I already pointed out the same problems addressed by Nielsen (BZN 70: 157–159) in my nomenclatural review of the Phylum Phoronida for the respective volume of the *Fauna Ibérica* series (Alonso-Zarazaga, 2006). However, my comments seem to have been overlooked when preparing this application, maybe because of being written in Spanish (although most volumes of this series have nomenclatural comments and acts in the Appendix). They were an answer to the nomenclature proposed by the authors of the taxonomical part (defended by Emig et al., 2006, pp. 54–56), which was not Code compliant for the same reasons exposed by Nielsen in Case 3626 (parallel nomenclatures for adults and larvae, and application of a so-called 'status quo' derived from Silén's (1952) 'unofficial proposal'). I commented on the disqualification using 'ad hominem' arguments of the names and nomenclatural propositions presented by Dalla Torre (1889) and Poche (1903): they could not write on Phoronida because they were not specialists in this group. I also criticized the wrong use the authors made of several articles of the Code (namely 23.2 and 23.9.2) to support their

incorrect nomenclature. Moreover, they argued [my translation from Spanish]: ‘... in the present hierarchy of Phoronida, no family has ever been described and there is no available diagnosis for this rank.’ I showed then in my answer this was a false assertion, and I will give more data on this point below.

Nielsen (BZN 70: 158, para. 7) recognizes the existence of a family PHORONIDAE, attributing it to Hatschek, 1888, as for the class Phoronida, the only taxon described by this author. Whereas the latter is true (for the class only), the first is not, and the author has missed three other available names. More information on higher taxa names intended for *Phoronis* and its allies (Phoronaria Haeckel, 1896, Phoronia Haeckel, 1896, Actinotrochoidea Poche, 1908, Vermiformiae Délage & Hérourard, 1897, Phoronidea Lang, 1888, Actinotrochidea Poche, 1908 and Diplochorda Masterman, 1896) is available in Alonso-Zarazaga (2006).

The following names have been proposed for a family in Phoronida:

1. PHORONIDAE Hatschek, 1881 (p. 72), incorrectly given as of 1880 in Alonso-Zarazaga (2006, p. 209). This name has no description but it is available by indication by virtue of Article 12.2.4, even if the selected stem is incorrect. The name *Phoronis* comes from the Greek proper noun φορωνίς (an eponym of Io), genitive φορωνίδος, whose Latinized stem is Phoronid-.

2. PHORONIDAE Czerniavsky, 1881 (p. 287). This taxon is described as new and has a short description, it is available as well. I do not know the relative precedence of this and the previous name.

3. PHORONIDIDAE Dalla Torre, 1889 (p. 90). This name is correctly formed, and, since no author is mentioned, it is best considered to be a subsequent spelling of PHORONIDAE.

4. ACTINOTROCHIDAE Poche, 1903 (p. 466). An available name based on *Actinotrocha* Müller, 1846.

I consider advisable that the author of Case 3626 completes his application by requesting the placement of ACTINOTROCHIDAE Poche, 1903 in the Official Index of Rejected and Invalid Family-Group Names in Zoology and by requesting as well the placement in the Official List of Family-Group Names in Zoology of the name PHORONIDAE, with the appropriate authorship, to have the spelling fixed. I understand that this spelling is in prevalent usage and should not be modified to PHORONIDIDAE, under the provisions of Article 29.3.1.1.

And finally, Nielsen’s designation of type species for *Phoronis* (BZN 70: 157, para. 2) is invalid, since there is at least one previous designation (Emig et al., 2006, p. 39) for the same species, *P. hippocrepi* Wright, 1856. Consequently, I request the Secretariat of the ICZN to modify the wording of Nielsen’s application in para. 10 (2) to read as follows:

(2) to place on the Official List of Generic Names in Zoology the name *Phoronis* Wright, 1856 (gender: feminine), type species by subsequent designation by Emig, Roldán & Viéitez (2006) *P. hippocrepi* Wright, 1856.

Additional references

Alonso-Zarazaga, M.A. 2006. Apéndice 1. Nomenclatura: Lista de sinónimos y combinaciones filo *Phoronida*. Pp. 209–213 in Alvarez, F., Emig, C.C., Roldán, C. and Viéitez, J.M. *Fauna Ibérica*, 27. 276 pp. Museo Nacional de Ciencias Naturales, CSIC, Madrid.

- Czerniavsky, V.** 1881. Materialia ad zoographiam ponticam comparatam. Fasc. III. Vermes. Materialy dlB sravnitel'noj zoografii Ponta. Vyp. III. Červi. *Bulletin de la Société Impériale des Naturalistes de Moscou*, **55**(2)(4) [1880]: 213–363.
- Dalla Torre, K.W. von.** 1889. Die Fauna von Helgoland. *Zoologische Jahrbücher, Supplementheft 2*: 1–99.
- Hatschek, B.** 1881. Ueber Entwicklungsgeschichte von Echiurus und die systematische Stellung der Echiuridae (Gephyrei chaetiferi). *Arbeiten aus dem Zoologischen Institut der Universität Wien und der Zoologische Station in Triest*, **3** [1880]: 45–78 [1–34].
- Emig, C.C., Roldán, C. & Viéitez, J.M.** 2006. *Phoronida*. Pp. 19–56 in Alvarez, F., Emig, C.C., Roldán, C. and Viéitez, J.M. *Fauna Ibérica*, 27. 276 pp. Museo Nacional de Ciencias Naturales, CSIC, Madrid.
- Poche, F.** 1903. Über den richtigen Namen der Gattung Phoronis Wright. *Zoologischer Anzeiger*, **26**: 466–467.

Comment on the proposed conservation of *Kalophrynus* Tschudi, 1838 (Amphibia, Anura, MICROHYLIDAE) by designation of a neotype for its type species *Kalophrynus pleurostigma* Tschudi, 1838

(Case 3618; see BZN **70**: 86–88, 205)

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While we agree with Bouchet's desire to select a museum voucher from which molecular data can be obtained, or for which such data have been obtained and deposited in GenBank, what is desirable in the designation of a neotype is not always possible. Museum specimens of *Kalophrynus pleurostigma* are rare in natural history collections. Currently, no sequences for Sumatran *K. pleurostigma*, which would be from the same island as the original holotype, are available in GenBank. In fact, only four *Kalophrynus* are listed in GenBank: two pet-trade specimens without locality data; one from central Thailand; and the fourth from northern Myanmar. Potentially, the early collection date of the proposed neotype (the year 1905) might permit DNA extraction. At that time, herpetological specimens were still commonly preserved in alcohol, because formalin had not yet become the standard preservation fluid. In the absence of Sumatran material of *K. pleurostigma* that has associated sequences, we conclude that our choice of neotype is sound.

Comment on *Terrapene putnami* Hay, 1906 (Testudines, EMYDIDAE): replacement of the holotype by designation of a neotype
(Case 3628; see BZN 70: 193–198)

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We write in support of the proposal to replace the existing holotype of *Terrapene putnami* Hay, 1906 with a neotype as proposed by Ehret (BZN 70: 193–198). Single plastral elements are not particularly diagnostic at the species or genus level and as such do not give adequate material for comparative morphology. In fact most of the diagnostic morphological characters available in turtles require carapace elements (Thomson & Mackness, 2000; Thomson, 2000) and skulls (sensu Gaffney, 1979). For these reasons replacing the undiagnostic holotype with a neotype that is diagnostic is desirable for both nomenclatural and taxonomic reasons.

Case 3628 has clearly outlined the nomenclatural issues with variable applications of the name and uncertainty on how to apply it in relation to both other fossil forms and the living forms of the genus *Terrapene*. In a large and diverse group of species it is unfortunate and inconvenient to be unable to properly allocate names that are available and valid. From a taxonomic point of view it is difficult to propose new combinations or new species without any certainty of where those names already published should be utilized. This becomes a negative impact in that it discourages people from proposing new arrangements because of a fear of an unstable nomenclature.

Therefore, we strongly support the proposal by Ehret to replace the existing holotype (AMNH 6097) with the suggested neotype (UF 3066). The aim of this would be to stabilize the nomenclature of *Terrapene putnami* Hay, 1906.

Additional references

- Gaffney, E.S.** 1979. Comparative cranial morphology of recent and fossil turtles. *Bulletin of the American Museum of Natural History*, **164**(2): 65–376.
- Thomson, S.** 2000. The identification of the holotype of *Chelodina oblonga* (Testudines: Chelidae) with a discussion of taxonomic implications. *Chelonian Conservation and Biology*, **3**(4): 745–749.
- Thomson, S.A. & Mackness, B.** 2000. Fossil turtles from the early Pliocene Bluff Downs Local Fauna, with a description of a new species of *Elseya*. *Transactions of the Royal Society of South Australia*, **123**: 101–105.

Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published

(Case 3601; see BZN 70: 234–237)

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Case 3601 seeks to perpetuate false nomenclature. Those unfamiliar with the controversy over Raymond Hoser's taxonomic contributions to herpetology should take a look at several issues of the *Australasian Journal of Herpetology* (hereafter, *AJH*; available through the website www.smuggled.com/AJHIP1.htm) as well as associated webpages (see the list at www.smuggled.com/faq1.htm) so that they can better appreciate the situation he has created for herpetologists. These names place a significant burden on herpetological nomenclature and, as of this writing, add up to 604 taxon names beyond *Spracklandus*, across all groups of reptiles. Hoser produces taxon names by the dozen in a manner that he proclaims to be compliant with the Code yet which are clearly crafted without the constraints of due scientific process, thus failing to meet the criteria of Article 8.1.1 of the Code (a work 'must be issued for the purpose of providing a public and permanent *scientific* record'; emphasis added).

One may ask how it is even possible that one author, working without examining museum specimens or input from experts in the field and generating insufficient data, produces so many taxonomic decisions across such a wide taxonomic arena in such a short period of time (2012: $n = 280$; 2013: $n = 255$). Examination of the issues of *AJH* shows the pattern: start with one very basic taxon naming section devoid of sections on methodology, specimen lists, new data, original interpretations or illustrations, which is filled with a single text block that includes all the literature on the particular group available; then, after copying and pasting as needed, the listing of literature is changed as appropriate for each treated group, specimens are picked from the lists of others when needed, and an extensive etymology is composed. As a consequence, Hoser's taxon names, *Spracklandus* among them, are almost entirely dubious in their inception, and it is no wonder that this methodology has been heavily and formally criticized in many publications (e.g. Aplin, 1999; Wüster et al., 2001; Borrell, 2007; Wallach et al., 2009; Zaher et al., 2009; Bates et al., 2013; Kaiser, BZN 70: 293–302, December 2013), and by the herpetological community at large (Kaiser et al., 2013).

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The genus name *Spracklandus* Hoser, 2009, was clearly presented chronologically ahead of *Afronaja* Wallach et al., 2009. Therefore, should its publication be judged to be Code-compliant, there is no argument regarding Article 23 (the Principle of Priority). However, given that serious questions were, and continue to be, raised

regarding the circumstances under which this particular issue of the *AJH* was published (Wallach et al., 2009), a close examination of the facts is in order.

After a review of photographs of the copy of Issue 7 of the *Australasian Journal of Herpetology* held by the Australian National Library, which is unquestionably an original copy of the document under scrutiny, it becomes clear that this document does not meet the requirements of Article 8.1.3. Based on the photographs, the following can be stated:

(1) As clearly visible on the first page (Fig. 1A), there is a printer-produced pattern embedded in the black emblem. This pattern is also easily visible on p. 12, which has white writing on a black background. In a normal printing company run of 100 copies or more, such a pattern would be detected as part of the regular quality-control process and suitable adjustments would be made. However, if someone were to home-print individual double-sided copies, as appears to have been the case here, such a pattern may not be detected. A similar almost identical ink pattern is visible on the single-sided copy later received by Van Wallach (Fig. 1B). I believe this shows that there really was no print run of 'numerous identical and durable copies' (Article 8.1.3), as Hoser asserts.

(2) With an ink defect present on a document, such patterns will vary slightly from copy to copy, meaning that it is not possible to produce visually identical copies. Furthermore, the online issue includes colour in its layout, whereas the printed copies are black-and-white with grayscale images. While I think the spirit of the Code should be interpreted here to mean 'identity of content,' I feel it is prudent to include all details.

(3) The position of the staple in the upper portion of the document (Fig. 1C), horizontal near the top of the page and not in the upper left hand corner, as Hoser claims, shows once more that this document was not produced in an edition and that Hoser himself is no longer sure how he produced 'original copies.' There is no printing machine that places staples in the position where these original staple holes are (the library appears to have re-stapled the pages in the exact location of the original staple). Incidentally, the staple in Wallach's copy is vertical along the left margin in the upper left hand corner of the page (Fig. 1D).

(4) The presentation of this work does not reflect the level of durability expected from a 21st Century work compliant with Article 8.1.3. If someone were to request a 'durable copy' of a given document and then received what we can see in the images, I contend that this would be unacceptable. The hallmark of a 'durable' item is that it can withstand repeated handling and the test of time. If this document were to be handled frequently, even if only to open it for reading, there are potential problems with the fastening and the paper itself (showing some fraying after only a few years in a library).

(5) Based on the condition of the copy in the Australian National Library, which all acknowledge is currently the only accessible original copy of this issue, there can be no doubt that the work was printed on a desktop printer and hand-stapled. While the printing medium itself may conform to the Code, much of the initial production of Issue 7 clearly does not. Furthermore, I have seen no proof that there were ever more than a handful of copies produced around the publication date (receipts confirmed only for the Australian National Library, *Zoological Record*, and Robert Sprackland).

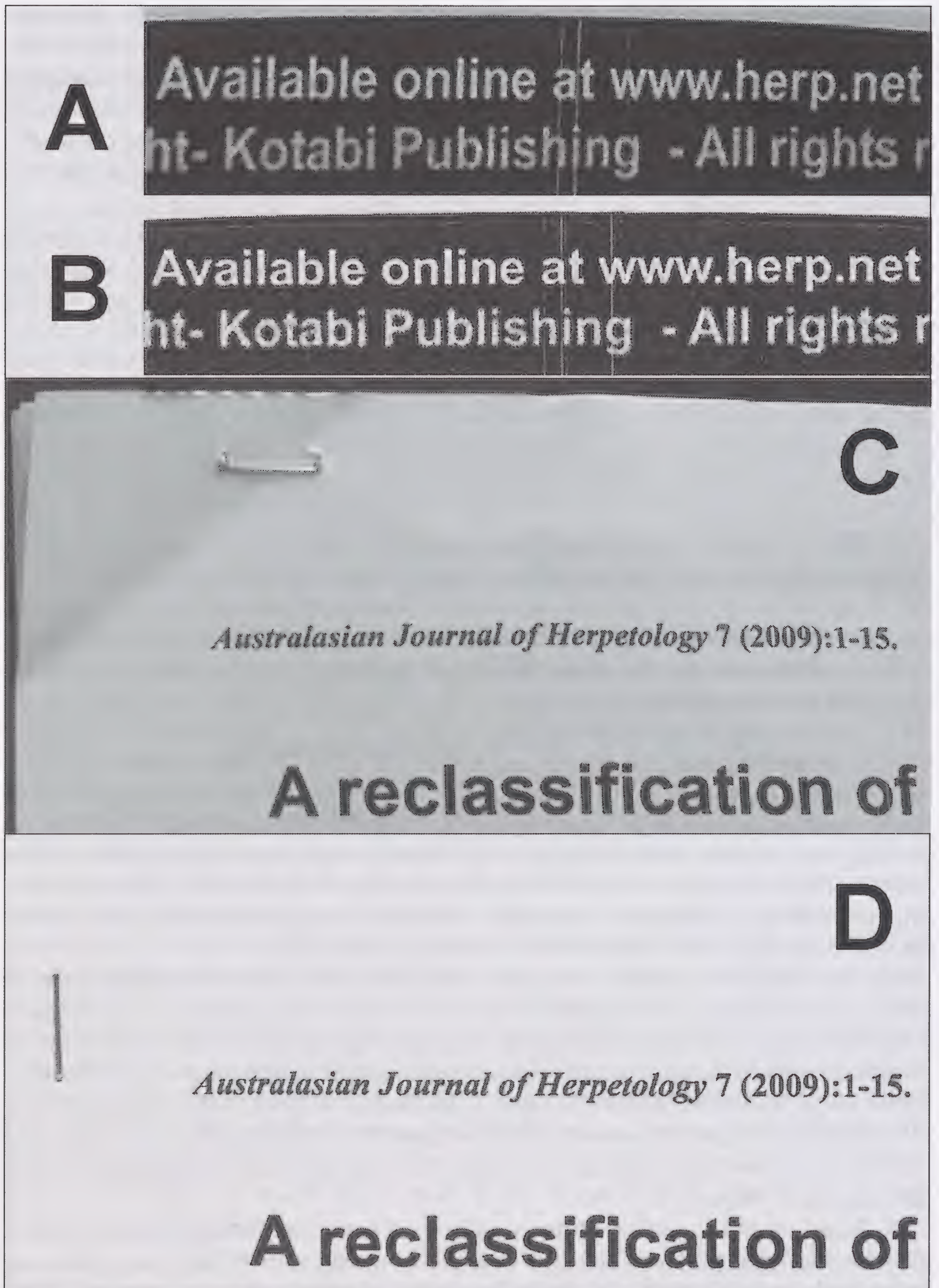


Fig. 1. Details of an original (A, C) and a Van Wallach's copy (B, D) of Issue 7 of the *Australasian Journal of Herpetology*. (A, B) The streaking in the ink running through the logo is very similar, and was probably caused by a worn print roller. (C, D) The position of the staple in (C) demonstrates that the original was hand-stapled. The position and direction of the two staples is different. (A, C) From photographs by Phil May. (B, D) Scans provided by Van Wallach.

I conclude that in addition to violating Article 8.1.1 this work contravenes four tenets of Article 8.1.3. (i) The work cannot be considered as having been published ‘in an edition,’ in the usual meaning and understanding of this word; (ii) there is no evidence that ‘numerous’ copies were made, as ‘numerous’ is commonly understood to mean ‘great in number, many’; (iii) the copies are not ‘identical’; (iv) the copies are not ‘durable’ in the commonly accepted meaning of the word. Therefore this work is not Code-compliant and appears instead to conform to the description in Article 9.12 of the amendment to the Code (ICZN, 2012; formerly Article 9.7) for an item explicitly considered unpublished by the Code. Given that, for decisions relating to the availability and priority of names, key articles of the Code must be adhered to, this work fails several critical aspects. Therefore, taxon names based on taxonomic decisions presented in Issue 7 of *AJH* must be excluded from zoological nomenclature. It also appears to have been the intent of the author to validate the nomenclatural availability of the entire run of the *AJH* (see the title of Case 3601), although the Editor has assured me that such a request was not intended and cannot be part of the Commission’s voting.

A Momentous Decision

The Commission has now been asked to rule on the proposals in Case 3601. I have previously proposed in the pages of this journal (Kaiser, BZN 70: 293–302) that taxon names produced outside of scientific process after the year 2000 (i.e. in violation of the Best Practices proposed by Kaiser et al., 2013) should be considered non-existent for the purposes of nomenclature. If this proposal were to be accepted by the Commission, such names, including *Spracklandus*, would fall outside of the scope of the Code, and the Commission could then formally reject the Case as being outside its jurisdiction, now that it has been formally presented.

I have also argued that the presentation of pseudoscience is but one of many ethical problems besetting science in general and taxonomy in particular (Kaiser, BZN 70: 293–302). While I do not dispute that a wide variety of transgressions against generally accepted scientific norms or ethical scientific conduct occur throughout the sciences, I contend that the problem of errant taxonomy occupies a unique place. Unlike in non-taxonomic situations, where the scientific community can quickly and informally discredit and ignore bad science and freely condemn misconduct, taxonomists are restricted in their response because a formalized set of rules exists in the form of the Code, and because dealing with bad science and misconduct may, as in this case, require an interaction with a council of peers, the Commission. As stated by Dayrat (2005, p. 410), ‘The current codes make taxonomy a peculiar discipline: all taxonomic work is permanent, regardless of its scientific rigor.’ The impact of this unique, Code-generated situation is that the strict application of the Principle of Priority without regard for other factors requires scientists to honour the output of substandard works that would be ignored in other disciplines, while simultaneously incentivizing those seeking scientific immortality without scientific accomplishment to abuse the system.

It may be instructive to investigate possible outcomes of Case 3601, and how the scientific community and the public will perceive them. If the Commission rules in favour of the case, then two taxonomies will emerge in herpetology, one system created, supported, and used by the herpetological community working according to

scientific Best Practices (as formalized through the votes taken by several major herpetological societies; see Kaiser et al., 2013), and one dissident system created by a single person, demonstrably not based on rigorous taxonomic research. The presence of two mutually exclusive taxonomic systems based on completely different premises will doubtlessly result in confusion among users, and it may lead to perpetual nomenclatural instability. It may also lead to the perception that there is a schism in the system, pitting those who uphold the Code in a supportive role for scientific taxonomic principles against those who uphold the Code as a pure, standalone entity unencumbered by those principles. Let me be clear: the current edition of the Code gives the Commission the power to set aside any provision of the Code in the pursuit of stable nomenclature (Article 81 of the Code). If the Commission rules against Case 3601, this would show that nomenclatural stability trumps taxonomy rejected by the herpetological community. The Commission could then also respond favourably to a case brought before it to suppress the *AJH* by using its plenary power, because this would align the trajectory followed by the herpetological community with the Code, avoid the potential for nomenclatural instability, and place those wishing to work outside of scientific principles and the Code of Ethics, on notice that the scientific community will not accept their involvement in taxonomy and the resulting nomenclature unless their taxonomic decisions are produced in accordance with scientific principles (scientific Best Practices). A ruling by the Commission merely to satisfy the Principle of Priority, in my opinion, would constitute too narrow an application of the Code to an issue that ultimately is much broader than the question of what to do with the genus name *Spracklandus*. In the interest of long-term stability in herpetological taxonomy, I believe it is time for the Commission to officially discard its policy of neutrality towards the merit of taxonomic decisions (see Harvey & Yanega, BZN 70: 216–217), and, as it begins to deliberate on Case 3601, I urge the Commission to join the worldwide herpetological community in opposing this flawed work.

The International Commission on Zoological Nomenclature is accordingly asked to:

- (1) confirm that Issue 7 of the *Australasian Journal of Herpetology* was not Code-compliantly published, failing to meet the criteria set forth in Article 8.1.1 of the Code;
- (2) confirm that Issue 7 of the *Australasian Journal of Herpetology* was not Code-compliantly published, failing to meet the criteria set forth in Article 8.1.3 of the Code;
- (3) place the name *Spracklandus* Hoser, 2009 on the Official Index of Rejected and Invalid Generic Names in Zoology.

Additional references

- Aplin, K.P. 1999. “Amateur” taxonomy in Australian herpetology—help or hindrance. *Monitor*, 10: 104–109.
- Bates, M.F., Tolley, K.A., Edwards, S., David, Z., da Silva, J.M. & Branch, W.R. 2013. A molecular phylogeny of the African plated lizards, genus *Gerrhosaurus* Wiegmann, 1828 (Squamata: Gerrhosauridae), with the description of two new genera. *Zootaxa*, 3750: 465–493.
- Borrell, B. 2007. The big name hunters. *Nature*, 446: 253–255.

- Dayrat, B.** 2005. Towards integrative taxonomy. *Biological Journal of the Linnean Society*, **85**: 407–415.
- Kaiser, H., Crother, B.I., Kelly, C.M.R., Luiselli, L., O’Shea, M., Ota, H., Passos, P., Schleip, W.D. & Wüster, W.** 2013. Best practices: In the 21st Century, taxonomic decisions in herpetology are acceptable only when supported by a body of evidence and published via peer-review. *Herpetological Review*, **44**: 8–23.
- Williams, D.J., Wüster, W. & Fry, B.G.** 2006. The good, the bad and the ugly: Australian snake taxonomists and a history of the taxonomy of Australia’s venomous snakes. *Toxicon*, **48**: 919–930.
- Wüster, W., Bush, B., Keogh, J.S., O’Shea, M. & Shine, R.** 2001. Taxonomic contributions in the “amateur” literature: comments on recent descriptions of new genera and species by Raymond Hoser. *Litteratura Serpentina*, **21**: 67–79, 86–91.
- Zaher, H., Graziotin, F.G., Cadle, J.E., Murphy, R.W., Moura-Leite, J.C. & Bonatto, S.L.** 2009. Molecular phylogeny of advanced snakes (Serpentes, Caenophidia) with an emphasis on South America xenodontines: a revised classification and descriptions of new taxa. *Papéis Avulsos de Zoologia*, **49**: 115–153.

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1. In his submission to the Commission, Hoser seeks to not only to have the generic name *Spracklandus* Hoser, 2009 conserved for a group of African cobras by the Commission but implicitly asks the Commission to decide whether or not his Issue 7 (2009) of his self-published journal, the *Australasian Journal of Herpetology (AJH)*, fully complies with the Code thus making several names and nomenclatural acts published therein available. However, there are several problems with this journal and specifically with the issue concerned. I therefore advocate the suppression of the name *Spracklandus* Hoser, 2009 and the placement of *AJH* on the Official Index of Rejected and Invalid Works in Zoological Nomenclature.

2. *AJH* is a self-published journal of which Raymond Hoser is the publisher, editor and, since its founding in 2009, the exclusive author. Within three months of the first publication, seven issues of *AJH* were produced naming 14 species and subspecies and 3 genera and subgenera, including *Spracklandus* Hoser, 2009. The existence of this outlet was primarily proclaimed in herpetoculture internet forums, and zoologists unlikely to participate in such forums were widely unaware of its existence (see the Code, Appendix B.8, General recommendations).

3. Article 8.1.1 of the Code states that works ‘... must be issued for the purpose of providing a public and permanent scientific record’. Given that publishers, editors and the scientific community as whole make great efforts to retain the integrity of the scientific record by preventing inadequate or unethical works to enter, Article 8.1.1 implies that works must have been produced in a way that enables them to enter the scientific record. Thus, works can only comply with this article if they also comply with the generally agreed and most basic standards in scientific writing, and hence are adequate to make a meaningful contribution to the scientific record. Adherence to these standards lies within the responsibility of authors, editors, and publishers, with the latter two functioning as gatekeepers of the scientific record. Contrary to this, works of poor science, little scientific merit, or produced in violation of scientific principles do not qualify to enter the scientific record and should be rejected immediately by an independent editorial board.

4. Case 3601 states that Issue 7 of *AJH* was made available on 23 March 2009 but parts of the original print run had been distributed a few days earlier to a small group of institutions and individuals. This statement must be seen as evidence for the existence of paper copies, and therefore Article 21.4 of the Code, 'Date incorrect' applies and the publication date must be advanced to the date of the first distribution (see Glossary of the Code for 'date of publication'). However, on the date the issue was distributed, it was not obtainable by the public. The Code explicitly does not recommend the distribution of original works on other than the specified date. Recommendation 21A of the Code states that an author, editor or publisher 'should not publish, permit to be published, or distribute a work, in whole or in part, for the first time other than on the specified date of publication. . .'.

5. In regard to the first seven issues of *AJH*, it is evident that these issues were produced by printing files on a domestic printer rather than having been professionally produced. While this itself does not render the status of the work noncompliant with the Code, it is impossible to determine the original source from which the printout was generated because both the paper and the online editions include the ISSN for both versions. In the absence of evidence to the contrary, it is not possible to determine whether or not the copies were printed in accordance with Article 8.1.3 or 'printed on demand.' The latter would be explicitly excluded by Article 9.7. One of the underlying principles of the Code is to '...effectively ensure that, irrespective of when and where they were published, names and the descriptions of new taxa would be permanently accessible and could be consulted most easily; moreover, there would be no doubt as to whether any name had been publicly presented in a form identical to all zoologists. . .' (the Code, Introduction: Development and underlying principles). Although the introduction is not a mandatory part of the Code, it reflects the spirit of the Code and helps to interpret the meaning of its provisions. Taken together the above evidence suggests that *AJH* must be considered as not published and the names presented therein must be considered de facto non-existent for the purpose of zoological nomenclature.

6. If the Commission, however, were to vote in favor of Case 3601 and declare the name *Spracklandus* Hoser, 2009 available, the Commission would thereby compromise the scientific record by opening a backdoor for works not published in adherence to scientific principles to enter the scientific record. This would be an inappropriate action by the Commission and might thereby diminish the influence of the Code in terms of its use in zoological taxonomy and generate user nomenclature that deviates from that compliant with the Code, causing even more confusion and nomenclatural instability. Very few zoologists will readily use the scientific names and concepts coined in the pages of *AJH*. I predict that the majority of herpetologists will follow the recommendations of Kaiser et al. (2013) and continue to ignore *AJH* as a reliable source for nomenclatural and taxonomic information.

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In March 2009, Raymond Hoser published Issue 7 of the *Australasian Journal of Herpetology* (hereafter *AJH*), of which he was then, and has remained since, the sole editor and sole contributing author. In this issue, he proposed the genus *Spracklandus* for the African spitting cobras (type species *Naja nigricollis* Reinhardt, 1843).

At the time of publication of Issue 7 of the *AJH*, Wallach and others were working on a manuscript detailing the division of *Naja* into four subgenera, *Naja*, *Boulengerina*, *Uraeus*, and a new subgenus, *Afronaja*, for the African spitting cobras.

On the *AJH* website, Hoser claimed the availability of a printed version of the journal free of charge at the time of the publication of Issue 7. This changed to a substantial fee shortly after publication of that issue in 2009. Ordinarily, it would be normal practice to assume journal publisher statements of this nature to be correct, however previous experience with this publisher led us to question the wisdom of relying on this assumption. Consequently, Wallach and others made enquiries with Australian libraries (through the Libraries Australia search system of the Australian National Library, which searches all major Australian libraries) and colleagues in Australian museums who we expected would have seen or received hard copies of the journal if indeed they existed. Our enquiries revealed a single hard copy, registered in the Australian National Library, Canberra. The second Australian library copy mentioned by Hoser (BZN 70: 234–237, December 2013), the State Library of Victoria, confirmed that its hard copy Issue 7 of the *AJH* was only received on 28 October 2009, i.e. after the publication of Wallach et al. (2009). This therefore does not constitute evidence for the existence of multiple copies at the time of the original publication. Since the copy Hoser sent to Van Wallach upon his request showed evidence of having been printed on demand, we concluded that there was no evidence to suggest the existence of a hard copy journal compliant with the requirement of Article 8.1.3. of the Code that ‘it must have been produced in an edition containing simultaneously obtainable copies by a method that assures numerous identical and durable copies.’ In the absence of clear evidence of *Spracklandus* being published within the meaning of the Code, Wallach et al. (2009) proposed the subgenus *Afronaja* for the African spitting cobras (type species *Naja nigricollis* Reinhardt, 1843), and considered the name *Spracklandus* to be unpublished.

Following the publication of Wallach et al. (2009), Hoser made representations to the editors of *Zootaxa* regarding the priority of his genus *Spracklandus*. He was invited to submit a rebuttal of Wallach et al. on three separate occasions by *Zootaxa*’s subject editors David Gower and Aaron Bauer, and Editor-in-Chief Zhi-Qiang Zhang, but failed to submit a manuscript to the journal (D. Gower, pers. comm.).

We maintain that Issue 7 of the *AJH* cannot be considered published within the meaning of the *Code*. Article 8.1.3, as in force in 2009, specifically required that any new name ‘must have been produced in an edition containing simultaneously obtainable copies by a method that assures numerous identical and durable copies.’ In our view, any publication ‘held together with a staple at the top left corner’, as described by Hoser (2013b) for Issue 7 of the *AJH*, fails the requirement of durability specified by Article 8.1.3; such documents are likely to fall apart with minimal handling. In this context, we also note that Recommendation 8 of Appendix B of the *Code* firmly places the responsibility for ensuring that new names are ‘self-evidently published’ on the author(s) of the names.

Finally, we submit that Hoser’s case needs to be assessed not solely on its own technical merits, but against the wider background of a very large number of poorly based names introduced by Hoser (Kaiser et al., 2013; Kaiser (BZN 70: 293–302, December 2013). The over 500 names (Kaiser et al., 2013; Kaiser (BZN 70: 293–302)) proposed by Hoser have been criticized by numerous authors (Aplin, 1999; Bates et al., 2013; Branch in Li Vigni, 2013; Kaiser et al., 2013; Schleip & O’Shea, 2010; Williams et al., 2006; Wüster et al., 2001; Zaher et al., 2009).

The point of view proposed by Kaiser et al. (2013), that these names should not be considered part of the scientific record, has received support from numerous individual herpetologists and most major scientific herpetological societies, including the World Congress of Herpetology. A Commission Opinion favouring Hoser’s case will place the Commission and the *Code* at odds with the clearly stated wishes and practices of the scientific herpetological community, and carries the risk that the authority and universal acceptance of the *Code* will be undermined.

The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to confirm that Issue 7 of the *Australasian Journal of Herpetology* is not published in the sense of the *Code* as a result of failing to meet the criterion of durability of Article 8.1.3;
- (2) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the name *Spracklandus* Hoser, 2009;
- (3) to place on the Official Index of Rejected and Invalid Works in Zoological Nomenclature Issues 1–21 of the *Australasian Journal of Herpetology*.

Additional references

- Hoser, R.T. 2009. A reclassification of the true cobras; species formerly referred to the genera *Naja*, *Boulengerina* and *Paranaja*. *Australasian Journal of Herpetology*, **7**: 1–15.
- Hoser, R.T. 2012a. Exposing a Fraud! *Afronaja* Wallach, Wüster and Broadley 2009, is a junior synonym of *Spracklandus* Hoser 2009! *Australasian Journal of Herpetology*, **9**: 1–64.
- Hoser, R.T. 2012b. Robust taxonomy and nomenclature based on good science escapes harsh fact-based criticism, but remains unable to escape an attack of lies and deception. *Australasian Journal of Herpetology*, **14**: 37–64.
- Hoser, R.T. 2013a. The science of herpetology is built on evidence, ethics, quality publications and strict compliance with the rules of nomenclature. *Australasian Journal of Herpetology*, **18**: 2–79.
- Li Vigni, F. 2013. *A Life for reptiles and amphibians*, vol. 1. Edition Chimaira, Frankfurt a. M.
- Schleip, W.D. & O’Shea, M. 2010. Annotated checklist of the recent and extinct pythons (Serpentes, Pythonidae), with notes on nomenclature, taxonomy, and distribution. *ZooKeys*, **66**: 29–79
- Wallach, V., Wüster, W. & Broadley, D.G. 2009. In praise of subgenera: taxonomic status of cobras of the genus *Naja* Laurenti (Serpentes: Elapidae). *Zootaxa*, **2236**: 26–36.

Comment on the proposed conservation of usage of *Touit* G.R. Gray, 1855 and *Prosopeia* Bonaparte, 1854 (Aves, PSITTACIDAE)
(Case 3640; BZN 70: 245–248)

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This case has been submitted owing to a paper by Gregory & Dickinson (2012) and the fact that we, the authors, failed to dig deeply enough into the precise origin of the name *Pyrrhulopsis* Reichenbach, 1850. In the light of the deeper research by Schodde et al. (2013) we are happy to state that we support their application.

Comment on the proposed conservation of usage of CORCORACIDAE Mathews, 1927 (Aves) and the spelling *melanoramphos* Vieillot, 1817 for the valid name of the type species of its type genus
(Case 3630; see BZN 70: 238–244)

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I am in support of the proposal to conserve the family name CORCORACIDAE. By contrast I see no sufficient reason to abandon the original spelling *melanoramphos* in favour of *melanoramphos*. Granted it might be in prevailing usage. However, there is, I think, general agreement that the Glossary definition in the 1999 Code does not provide a clear and unambiguous methodology for determining prevailing usage. There is a need for such a methodology; however, I believe any debate on the subject should start from a re-examination of that need, and then examine whether the background has changed since the time when prevailing usage seemed like the only solution. I believe zoologists generally would agree that the original concept arose in the context of wholly different names when earlier applicable but forgotten names were being ‘rescued’ from synonymy. By contrast I think that the ‘mission-creep’ which has extended that original concept to one where minor spelling changes are seen in the same light was, and is, unfortunate. This is ever more true; the Biodiversity Heritage Library makes access to old works, and thus original spellings, more and more easy. Original spellings should be seen as the right basis for stability because they remain before us. As the Code now describes prevailing usage any declaration that a given spelling is in prevailing usage could be revised within a matter of years due to the ease of rediscovery of use of the original spellings. The relevance of ZooBank to this should be considered. Wherever possible changes to original spellings should be avoided and not inflicted on ZooBank with the requirement that the change be recorded therein. I am not suggesting that the Articles in the Code that

either mandate or permit changes should be ignored; plainly they should not. Nor do I have a clear preference for retaining or abolishing gender agreement although this, owing to taxa being reallocated between genera, has been shown to be the single greatest cause of spelling differences, and thus of claims of instability in relation to names of birds (Olson, 1987).

Additional reference

Olson, S.L. 1987. On the extent and source of instability in avian nomenclature, as exemplified by North American birds. *Auk*, **104**(3): 538–542.

Comment on *Grallaria fenwickorum* Barrera & Bartels, 2010 (Aves, GRALLARIIDAE): proposed replacement of an indeterminate holotype by a neotype
(Case 3623; see BZN **70**: 99–102, 256–269)

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We consider that the designation of a neotype for *Grallaria fenwickorum* Barrera & Bartels, 2010 is not necessary because the name is not available, i.e. the description by Barrera et al. (2010) does not satisfy criteria of availability for names published after 1999 because they failed to designate a holotype unambiguously (an explicit fixation is lacking). The Code requires type specimens to be explicitly and unequivocally designated when proposing new species-group names after 1999 (Articles 16.4 and 72.3) and, by definition, a holotype should be a single specimen (Article 73.1). The holotype designation by Barrera et al. (2010) contains a fundamental ambiguity. The designation is divided in two parts: ‘a’ and ‘b’. In part ‘a’, they designated a sample of 14 feathers as the holotype, whereas in part ‘b’, they designated a bird depicted in a photograph as the holotype (the photograph was published on the cover

of the same issue of the journal). The typification is ambiguous because it is not clear whether the holotype is the sample of feathers or the bird in the photograph. This ambiguity is not a lapsus in the wording of the type designation; instead, the ambiguity persists for the remainder of the article. For example, an entire paragraph is used to justify the sample of feathers as an appropriate holotype (p. 10) but the 'Description of the holotype' (p. 11) is entirely based upon the bird photographed, not the sample of feathers. Therefore, Barrera et al. (2010) intentionally designated two entities as the name-bearing type and used one holotype or the other alternatively throughout the description as a way to cope with different interpretations of the Code (acknowledged by Gonzalez et al., 2011).

A holotype can be a whole animal, or one or more parts of an animal, but it must be a single specimen derived from a single animal (Articles 72.5 and 73.1). In ornithology, the holotype is typically a preserved 'round skin' specimen, which is just a part of the original bird. Other parts from the same bird (tissue samples, partial skeletons, stomach, etc.) can also be part of a holotype (i.e. holotypes can be composed of multiple parts). However, the typification by Barrera et al. (2010) does not conform to a holotype composed of multiple parts for two reasons. First, the two 'parts' of the holotype were not treated as a single specimen. The feathers were preserved but the bird was not. Barrera et al. (2010) actually declared that they released the holotype back into the wild, a fact that was reaffirmed subsequently by one of the authors (ProAves, BZN 70: 256–269, December 2013) and documented with photographs published by González et al. (2011) and online (<http://www.flickr.com/photos/proaves/sets/72157623898966996/>). We interpreted this action as in direct contravention of Article 16.4.2, which requires a declaration regarding the deposition of the type specimen in a collection. According to other interpretations, Article 16.4.2 does not apply in this case: because the type was not preserved, it cannot be an 'extant specimen' (González et al., 2011). In any case, the fact that the two parts of the holotype were treated as different specimens remains clear.

Secondly, the evidence available indicates that the feathers and the photograph were not taken from the same individual bird; thus, the holotype is a composite of different individuals. The bird that was captured and its feathers sampled (hereafter specimen A, depicted in figure 1 of González et al., 2011, also available at <http://www.flickr.com/photos/proaves/sets/72157623898966996/>) is different from the bird depicted on the cover page of Barrera et al. (2010) also designated as holotype (specimen B). Specimen A was photographed in the hands of an investigator while being sampled on 11 January 2010, and shows a prominent metal band on the right foot, just before it was released (the bird was banded during the study); its bill is clean and looks straight (the culmen is decurved but the gonys is recurved, resulting in no overall curvature). Specimen B, on the other hand, seems to be a free-roaming bird; other than some disarranged feathers, it does not show any sign of being captured and studied; in particular, it does not have a metal band on the foot; its bill is more decurved than in specimen A, mostly the effect of a straighter gonys; its bill and feathers around the face look dirty. Another photograph of bird B is available on the Internet Bird Collection (IBC, <http://ibc.lynxeds.com/photo/urrao-antpitta-grallaria-fenwickorum/holotype-foto-grallaria-fenwickorum>), where it is labelled as depicting the holotype of *fenwickorum*; the bill of this bird shows blotches of dirt in exactly the same places as the bird in the cover of Barrera et al. (2010), suggesting that the two

photographs were taken at least on the same day. Although the cover photo was reportedly taken on 11 January 2010, this could not be confirmed independently, since the Exchangeable image file format (Exif) metadata of the digital file were erased. However, the IBC photo of specimen B was taken on 9 January 2010 (reported in the IBC site and confirmed by the Exif metadata). Therefore, specimens A and B not only look different and have signs of differential treatment, but they also were photographed two days apart. Finally, we noted that the biometric measurements reported for the holotype (Barrera et al., 2010, Table 1) do not coincide with the measurements taken when the bird was captured and banded on 11 January (see notebook depicted in the photographs in González et al., 2011, also available at <http://www.flickr.com/photos/proaves/4538313633/>). Overall, the evidence demonstrates that at least two individual birds were involved. Therefore, Barrera et al. (2010) simultaneously and intentionally designated two birds as ‘the holotype’, an action that invalidates the description since fixation of a single specimen as holotype is required for descriptions after 1999 (Article 16.4.1).

Several arguments have been presented in defence of the *fenwickorum* description. Those regarding the Principle of Priority will not be discussed here since this principle concerns available names, and we consider *fenwickorum* not available. Barrera et al. (2010, see also González et al., 2011) argued that because *fenwickorum* is based upon photographs, Article 73.1.4 applies (‘Designation of an illustration of a single specimen as a holotype is to be treated as designation of the specimen illustrated; the fact that the specimen no longer exists or cannot be traced does not of itself invalidate the designation’), and no preservation of type specimens would be necessary. However, the alluded photographs were never designated as holotypes; instead, the ‘individual depicted’ in the photographs was designated as holotype directly; therefore, Article 73.1.4 is irrelevant in this case. González et al. (2011, p. 50) tried to make the case that, because the bird sampled was not a holotype at the moment of study, Article 16.4.2 does not apply, and no preservation of the holotype would be required. However, it is evident that individual feathers were collected knowingly on 11 January 2010, indicating the intent of designating the specimen under study as the name-bearing type (ProAves, BZN 70: 263). Lastly, it has been argued that because types can be just parts of an animal, deposition of parts of a holotype is sufficient for the purposes of Article 16.4.2 (Barrera et al., 2010, González et al., 2011). Although a holotype can be any part of an animal, the holotype itself must be preserved, not just a fragment of the holotype.

For the reasons expressed above, we conclude that the name *fenwickorum*, Barrera & Bartels, 2010, is not available for nomenclatural purposes. Because another name is available and in current use for this bird, *Grallaria urraoensis* Carantón-Ayala & Certuche-Cubillos, 2010, described by the actual discoverers of the new species, the unavailability of *fenwickorum* does not result in any inconvenience or nomenclatural instability. Therefore, we think that no action from the Commission is required, other than clarifying matters publicly by placing *fenwickorum* on the Official Index of Rejected and Invalid Names in Zoology and *urraoensis* on the Official List of Specific Names in Zoology.

We also consider the comment on this case by ProAves (BZN 70: 256–269) to contain several fallacious and misleading statements regarding the history surrounding the descriptions of *G. fenwickorum* and *G. urraoensis*. However, we restrain from

setting the record straight here and restrict this comment to the nomenclatorial issues that the Commission is asked to consider. A full dissection of ProAves (BZN 70: 256–269) will be published elsewhere.

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Additional reference

González, J., Proctor, G. & Bruno, E. 2011. The nomenclatural availability of and priority between two recently described names for the same new antpitta species from Colombia. *Conservación Colombiana*, **15**: 45–54.